about twenty-five years ago, being a very fine collection of many thousand ornithological specimens, with a quantity of interesting correspondence with Mr. J. Thompson, of Knowsley Aviary, Mr. Reid, of Doncaster, R. Dunn, of Hull, and many other naturalists of that period; these explain upon what terms he obtained the egg and a very fine specimen of the Great Auk.

ALBERT F. CALVERT.

63, Patshull Road, Kentish Town, March 19.

Superheated Steam.

LORD RAYLEIGH (p. 438) rebuts my objection to the statement regarding the efficiency of a vapour-engine in which pure water is replaced by a saline solution, pointing out "that Maxwell's exposition of Carnot's engine applies *without the change of a single word*, whether the substance *in the cylinder* be water, mercury, or an aqueous solution of chloride of calcium." The latter italics are mine. In the statement objected to by me the aqueous solution of chloride of calcium was *in the boiler*, and what was *in the cylinder* was *superheated steam*, which is not included in the above list, so that the application of Maxwell's exposition is somewhat difficult. The greater part of the fresh water supplied to passengers in steamships is now produced by condensing the *superheated vapour of a saline solution*, and the culinary experience is that the substance which was in solution has all been left in the boiler. My contention, therefore, still stands—the saline mixture is not the working substance, and Carnot's law refers to the working substance only, and not to anything left in the boiler.

"In each case there is a definite relation between pressure and temperature." This is evidently merely a slip of the pen, the writer having for the moment forgotten that he was dealing with *superheated* steam, for which there is not a definite relation between pressure and temperature. The condition of superheated steam is completely defined when *both* pressure and temperature are given; but pressure is here a function of temperature-and-something-else, and temperature is here a function of pressure-and-something-else. That something-else may be volume or it may be energy, or, preferably, it may be entropy, but it must be something which cannot be predicated from pressure alone or from temperature alone.

"(So far as the substance is concerned), all that is necessary for the reversible operation of the engine is that the various parts of the working substance should be in equilibrium with one another throughout." No; for, in addition, it is necessary that the working substance should have only one pressure consistent with any given temperature. For this reason, superheated steam, however it may have been produced, can never be the working substance in a Carnot's engine. In the reversed cycle, when the steam is raised from a saline solution, from the beginning of the higher isothermal, the pressure would go on increasing until it became that due to saturated steam at the temperature of the superheat. This might be double the maximum pressure in the original cycle.

"The various parts of the working substance should be in equilibrium with one another throughout." The writer seems to say that the steam of a saline solution is a stable saturated vapour. It is H_2O at a given pressure and temperature, and the condition of the substance is by this definition completely determined, and there is no alternative; but it is not stable. Say that the steam-space of the boiler is increased by adding a vertical cylinder alongside the boiler, open to it. On the bottom of that vessel the steam might condense—pure water—and the temperature of the steam immediately over this water would be that of saturated vapour at the same temperature, and from there all through the steam-space to the surface of the saline solution in the boiler the temperature would increase, and all would have the same pressure. There would be mechanical equilibrium, but not thermal equilibrium.

"At the upper limit, *all* the heat is received at the highest point of temperature,"—but just as it would be if the evaporation were from a film of water upon a nearly bare combustion chamber crown. The plate is left in the boiler, and so is the salt, and in neither case would the steam exhibit a "state of things strongly contrasted with that which obtains when vapour rising from pure water is afterwards superheated." I have stated in my previous letter that the heat of evaporation is all *received* at identically the same temperatures as when it is raised from pure water at the same pressure, and the contrast is only as strong as that between occult and obvious. I have now shown that the vapours must be identical from either beginning; and unless each carried a certificate of birth, I do not now see how it would be possible to tell one from the other.

London, March 12. J. MACFARLANE GRAY.

Phoronomy.

I THINK it will be admitted by all, that precision of language is of great importance in scientific terminology; and the letter of Dr. Besant, which appeared in your issue of last week (p. 462), certainly suggests strong reasons for employing the word *phoronomy* in the place of *kinematics*.

The word may at first sight appear strange to the present generation of mathematicians; but if it becomes acclimatized, its employment will appear as natural as the phrases *kinetic* and *potential* energy, in the place of such meaningless phrases as *vis-viva* and *force-function*.

When the medical profession require a new word, they almost always have recourse to the Greek language; and mathematicians and physicists would do well to follow their example, and in cases of doubt or difficulty to consult some eminent classical scholar. I must confess, that I have no sympathy with the attempts, which have occasionally been made, to introduce short words of Teutonic origin into scientific nomenclature, as such words have always appeared to me to be singularly deficient in point. A good example of this is furnished by the word *spin*, which Clifford attempted to introduce in the place of the phrase *molecular rotation*. The latter phrase, although a little long, exactly expresses the idea which it is intended to convey, viz. that the molecules of the fluid possess a motion of rotation as well as a motion of translation. The word *spin*, on the other hand, does not express any such idea, but is strongly suggestive of the juvenile, though not altogether unscientific, pastime of spinning peg-tops.

A. B. BASSET.

322 Oxford Street, W., March 18.

I HAVE before me the second edition of F. Redtenbacher's "Principien der Mechanik und des Maschinenbaues" (Mannheim, 1859), of which the first section is entitled "Die Bewegung als Erscheinung (Phoronomie)." Whether the term occurs already in the first edition (1852), I cannot affirm, but I remember very well that Redtenbacher, in his lectures in Carlsruhe, in 1858, insisted upon that term being distinct from "Dynamik" and "Kinematik." I conclude, therefore, that the majority of the 786 students of that year—among them many foreigners—as also those of other years, were conversant with the term.

M. AM ENDE.

Westminster Chambers, 5 Victoria Street,
London, S.W., March 19.

SOME other correspondent is pretty sure to be mentioning that Mr. W. H. Besant will find in Kant's *Metaph. Anfangsgründe der Naturwissenschaft* all the authority he could desire for his proposed use of the word "phoronomy." Kant regularly uses the word in the sense of the later "kinematic"; and he was man of science enough to justify anyone in following his lead.

G. C. R.

The Tudor Specimen of Eozoon.

IN reference to the remarks made by Sir J. W. Dawson (NATURE, March 17, p. 461) on my paper on the Tudor specimen of Eozoon (*Quart. Journ. Geol. Soc.*, vol. xlvii. pp. 348–55), I should like to say that the whole point of that paper was that it was based on Sir J. W. Dawson's original type. The figure of this specimen has been repeatedly republished by Sir J. W. Dawson, and, in the absence of illustrations or details of other specimens from Tudor, upon its evidence alone rests the asserted occurrence of Eozoon in the Tudor limestone, and the great claims based thereupon. The value of other specimens from this locality was not rated very highly by Sir J. W. Dawson so recently as September 1888, when he remarked, "*Without additional specimens*,[1] and in the case of creatures so variable as the Foraminifera, it would be rash to decide whether

[1] And he previously refers only to "*the* specimen," "this very interesting specimen," "the fine specimen from Tudor," &c.

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the differences above noted[1] are of specific value."[2] I may add that I have recently seen the specimens of Tudor limestone exhibited in the Peter Redpath Museum, and my estimate of their value coincides exactly with that of Sir J. W. Dawson in 1888. As Sir J. W. Dawson most kindly promises his assistance to other workers, perhaps he would submit to some of them any specimens from Tudor which he regards as more conclusive than his original type.

It would seem rather unnecessary for anyone to trouble to infer from my paper that Sir J. W. Dawson has "regarded the Madoc and Tudor specimens as 'Lower Laurentian,'" when that is so directly stated by Sir J. W. Dawson in his description of his figure; viz. "Specimen of *Eozoon canadense* embedded in a dark-coloured homogeneous limestone occurring in the Lower Laurentian series at Tudor, Canada West" (*Quart. Journ. Geol. Soc.*, vol. xxiii. p. 265). J. W. GREGORY.

British Museum (Natural History), S.W.

The Theory of Solutions.

IN his last letter (NATURE, March 3, p. 415) Prof. Ostwald repeats his opinion that a theory is "a complex of laws, grouped around and derived from a main law," and infers from my letter that what I term a *theory* he would term an *hypothesis*.

If this were the whole point at issue, I could meet it in no better way perhaps than by referring Prof. Ostwald to his own works. For example, in his "Outlines of General Chemistry," are to be found not only numerous instances of the use of the word theory in its ordinary and accepted sense (*e.g.*, p. 58) but also cases in which it is employed as synonymous with hypothesis (*e.g.*, p. 187).

With regard to the definition of solutions as mixtures, Prof. Ostwald maintains that even if hydrates are formed in a solution, the solution is finally a mixture of the hydrates and the remaining solvent. The real question involved is unaffected by this explanation. There is no doubt whatever that to the majority of readers the definition, without any qualifying clause, that solutions are mixtures leads to one conclusion and no other—namely, that between solvent and dissolved substance there is no interaction of a chemical nature. Prof. Ostwald has in his letters stated that in some cases he considers such interactions occur; he has also stated that between chemical and physical processes he knows of no distinction. The definition is at variance with both these views, and it seems but fair to conclude that such discordant statements tend in no way to obviate that misconception which Prof. Ostwald so often deplores.

In defence of the application of van der Waals's equation to solutions, a process questioned by me in my letter, Prof. Ostwald states that van der Waals himself has taken up this very question. The method by which van der Waals approaches the subject, curiously enough, furnished the main grounds for my objections. The most superficial comparison of the complex formula which van der Waals deduces for a mixture of two substances, with such an application of his simple gas equation to a solution as that given in Prof. Ostwald's book, is ample justification for my strictures. But apart even from such evidence as to the inadequacy of the application, the form which it is finally made to assume is in itself a proof of its incompleteness. By judicious simplification the application is made to take the shape of a linear equation in which "pressure forces due to the interactions of molecules are absent." That is to say, the cohesion of solvent and dissolved substance, and the mutual reactions of both, are alike ignored. Further comment on such a method of accounting for the phenomena of solutions appears to me to be superfluous. J. W. RODGER.

London, March 7.

The Limpet's Strength.

THE limpet experiments of your esteemed correspondent, Mr. Percy Aubin, as reported in NATURE of March 17 (p. 464) would have been still more interesting and instructive had he weighed the animals deprived of their shells.

On April 10, 1890, I published my experiments showing that the shell-less limpet pulls 1984 times in the air its own weight, and about double when immersed in water.

Fasting fleas on an average pull 1493 times their own dead weight.

Other experimenters give the pulling power of the shell-deprived *Venus verrucosa* of the Mediterranean, a cockle-like creature, at 2071 times the weight of its own body.

The force required to open an oyster appears to be 1319·5 times the weight of the shell-less oyster.

J. LAWRENCE-HAMILTON, M.R.C.S.

30 Sussex Square, Brighton, March 19.

Technical Education for Novelists.

AMIDST the many schemes for technical education, could you not put in a plea for the "author of the popular novel"? Perhaps the need will best appear from these illustrations taken from the first 100 pages of a recently published and loudly heralded work.

(1) Scene—Kinder Scout, Derbyshire. Date—"after the snows and rains of early April," 1864. Time—*after* 8 *p.m.* "It was a clear, frosty night, *promising* a *full* moon."

(2) Same place, *Easter Eve*, 1864. "The wooded sides of the great moor were fading into dimness, and to the east a *young* moon was *rising*." W.

March 12.

THE ORIGIN OF THE YEAR.

I.

IT would seem that in the dawn of civilization it was not at all a matter of course that the sun should be taken as the measurer of time, as it is now with us; and in this connection it is worth while to note how very various the treatment of this subject was among the early peoples. Thus, for instance, it was different in Egypt from what it was in Chaldæa and Babylonia, and later among the Jews. In the Egyptian inscriptions we find references to the moon, but they prove that she occupied quite a subordinate position to the sun; while in Chaldæa it would seem that the moon was the chief thing worshipped, and it was thus naturally the chief means used for measuring time, and, so far as months were concerned, this, of course, was quite right. In Chaldæa, too, where much desert travel had to be undertaken at night, the movement of the moon would be naturally watched with great care.

An interesting point connected with this is that, among these ancient peoples, the celestial bodies which gave them the unit period of time by which they reckoned were practically looked upon in the same category. Thus, for instance, in Egypt the sun being used, the unit of time was a year; but in Chaldæa the unit of time was a month, for the reason that the standard of time was the moon. Hence, when periods of time were in question it was quite easy for one nation to conceive that the period of time used in another was a year when really it was a month, and *vice versâ*. It has been suggested that the years of Methuselah and other persons who are stated to have lived a considerable number of years were not solar years but lunar years—that is, properly, lunar months. This is reasonable, since if we divide the numbers by 12 we find that they come out very much the same length as lives are in the present day.

There seems little doubt that the country in which the sun was first definitely accepted as the most accurate measurer of time was Egypt.

"The Egyptians," says Ranke in the first chapter of his "Universal History," which is devoted to Egypt, "have determined the motion of the sun as seen on earth, and according to this the year was divided, in comparison with Babylon in a scientific and practically useful way, so that Julius Cæsar adopted the calendar from the Egyptians and introduced it into the Roman Empire; the other nations followed suit, and since then it has been in general use for seventeen centuries. The calendar may be considered the noblest relic of the most ancient times which has influenced the world."

[1] *i.e.* between *the* specimen from Tudor and those from other localities.

[2] "Specimens of *Eozoon canadense*," Mem. Peter Redpath Mus., 1888, p. 43.

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